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The Michel Benoist and Robert Mulholland yearly European Spine Journal review

A survey of the "surgical and research" articles in the European Spine Journal, 2005

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Introduction

This year has produced a great number of excellent papers that should have a significant effect on our approach to and management of a wide diversity of spinal problems. I hope that my comments on each paper will cause people to read or reread them. Do not just skim the abstract, as inevitably abstracts have to summarize the whole paper, and important conclusions in the paper itself may not surface in the abstract.

An important contribution that the European Spine Journal makes to surgical education and the daily practise of spinal surgery is the review article. They mainly deal with extreme topical issues**.** This year I particularly enjoyed those, which dealt with the nonoperative treatment of thoracolumbar burst fractures, Scheuermann's disease, adult scoliosis, and clinical studies in spinal surgery.

Despite reviewing some 2,700 references, and selecting some 17 eligible studies for review, the authors van der Roer et al. [27] claim that there was no scientifically sound evidence from high-quality randomized trials comparing the effectiveness of operative or conservative treatment of unstable traumatic thoracolumbar fractures, because such studies had not been done. However, this is a paper that deserves close study, as although it was my impression from the paper that the surgically treated had less pain in the long term, the lack of any clear difference may make surgeons considering surgery or not, to weigh both surgical factors and nonsurgical factors more carefully. Will a patient with a scar on his back and a plate perhaps extending to L3 be as good in

the long term compared with a patient with a satisfactorily healed single segment in some kyphosis? What is the social and psychological effect of having had a major operation on the spine, and the possibility of another operation to remove the plate?

The paper, which follows this review is by Agus et al. [3]. Although the study was performed with only 35 patients, the follow-up was for 6 years, and it was an excellent analysis of the results of treating conservatively, unstable burst fractures, which were neurologically intact. They discuss the concept of instability and using the Denis classification, treated both 2 column and 3 column fractures, but without facet fracture or facet dislocation. The short time of hospitalisation, only 3–8 days, and the use of bracing for comfort, not for maintenance of position, and the lack of any indication in the final results that kyphosis was a factor in clinical result, all rather undermines the usual suggested reasons for surgical intervention in unstable fractures. There was an increasing deformity at final follow-up, of kyphosis, and anterior and posterior vertebral height loss, but this does not affect the result, and as others have commented canal compromise dropped from around 45% average to 20% average, in both 2 and 3 column fractures. It would seem reasonable that in any general orthopaedic setting, without specialist spinal surgeons, a strong case can be made for conservative treatment of these fractures, as in relatively inexperienced hands surgery has the potential for significant complications.

Adult scoliosis

The review concerning adult scoliosis by Aebi [2] is a beautifully written and an important contribution to the surgery of the lumbar spine. As Aebi points out that with an aging but active population, the ''drift towards a grey society'', this disorder which was neglected in the past has now become an important part of adult low back surgery. These patients were presented to those surgeons dealing with low back pain. In the past such surgeons were not deformity surgeons, and the instrumentation available in the past was not appropriate for the difficult surgery required to correct deformity. The young spinal surgeon of today has the training to tackle these problems, and the instrumentation, and this article provides him/her with a comprehensive classification and understanding of the natural history that enables surgery to be contemplated with careful selection. I particularly liked Aebi's concept that ''as patients who present themselves with significant clinical problems in the context of adult scoliosis get older, minimal invasive procedures to address exactly the most relevant clinical problem may become more important, basically ignoring the overall deformity and degeneration of the spine''. Not only may the major surgery be difficult, (and in the older patient dangerous) but also the assessment and selection of appropriate intervention is difficult. Anyone contemplatively dealing with such patients will find in this review an excellent classification. There is a very comprehensive collection of the various types of deformity, how they may be investigated and managed, and if the decision is to surgically correct, then the problems that may be encountered are beautifully illustrated, and clear messages are given, such as if one is extending a fusion across the lumbo-sacral level, then that segment must be circumferential fused.

Scheuermann's kyphosis

The review dealing with Scheuermann's kyphosis by Arlet [5] was of value, as not only was it a comprehensive review of the literature, but also very practical surgical advice, both as regards decision about operative indications, but also technique. For those orthopaedic surgeons who do not operate on such patients, often the difficulty is to decide when they should be referred to a surgeon who will, this is often the most difficult decision, and this article provides the basis of knowledge to permit appropriate referral. The normal kyphosis in the adult is around 40°, and levels up to 70° may be a cosmetic problem, but not a pain problem. Once the deformity is 90° or more, then surgery should be considered. The advent of anterior thoracoscopic surgery which was dealt fully and clearly has made the surgery of Scheuermann's disorder safer and more effective, especially as it is now possible using these techniques to correct low down in the thoracic spine without taking the diaphragm down.

Clinical studies in spinal surgery: Hanson et al. [13]

This is a short review of a very complicated field, and perhaps of particular value to residents who may be taking a professional examination, and can be expected to be quizzed concerning the planning of research. The summary of the various types of study design and their particular value on occasion was refreshing in this era when the prospective randomized trial has become so established as the only type of study worthy of use. The view that a well-designed prospective cohort study may be a more practical type of study in surgery than attempting a blinded randomized study, which has such problems of recruitment is well stated.

Lumbar disc surgery and spondylolisthesis

One always enjoys papers that give a clear message, easily assimilated, especially if they confirm one's own prejudices. I enjoyed the two papers dealing with the

outcome after operations by Kara et al. [19] and Solberg et al. [32].

The first paper looked at 80 patients who had had an operation for a lumbar disc herniation, and looked at the characteristics of 34 of these patients in whom it was a second or third or even fourth operation. All patients were given the same instructions concerning exercises, but the group that had a further operation did not exercise as much and many of them did not have the habit of regularly exercising. The authors conclude that "lack of regular exercise is a significant risk factor for the development of lumbar disc degeneration." The confounding effects of other variables: age, BMI, gender, smoking, and occupation were small.

This paper empowers the surgeon in advising patients. How often after disc operations patients ask about the risks of a recurrence? The clear message that an early exercise regime and regular exercise appeared to be an important factor in preventing recurrence helps us advise our patients. Clearly some patients who are doomed to fail, may have too much pain to do an active exercise program, but clearly those who can, do better, so it is wrong to restrain patients after a disc operation to "be careful" or to restrict their physical activity.

The paper by Solberg et al. [32] is also being discussed by Dr. Benoist, but one point I would make is that I was surprised that the authors did not mention the imaging studies. Patients who seem sufficiently disabled to justify surgical treatment, and yet have a small protrusion do less well, and I was surprised that they did not address this (Spengler et al. [33]). The Spengler study using an objective score for imaging, physical findings, and psychological factors came up with the finding that positive imaging studies were indications of a successful outcome, and negative psychological factors were predictors of a poor outcome, even if the latter had positive imagine studies. The Solberg paper in describing the features of their poor results indicates the likelihood of psychological factors being present, much as Spengler indicates.

Another paper that dealt with the outcome was that by Rousseau et al. [28]. They conclude that result is better if an anterior fusion using a cage is added to the posterolateral fusion and decompression. In their circumferential fusion group with cage, the Beaujon score rose from 10 to 19, and in the posterolateral fusion group the change was 11.8–18. Using multivariate analysis they calculate that this is a significant difference, but is it significant enough to justify a circumferential fusion? However, they make a serious misquotation of the literature concerning the role of fusion in degenerative spondylolisthesis. In their discussion they state that "fusion itself is not automatically related to improved clinical outcome in degenerative lumbar spondylolisthesis" quoting Fischgrund's paper in 1997 [10]. In fact the Fischgrund paper was dealing with the issue of instrumentation or not, and there was no difference between an instrumented posterolateral fusion and one that was not instrumented, but both of Fischgrunds groups had fusions. The Herkowitz paper in 1991 [16] had established that fusion and decompression was better than decompression alone, it did not need to be an interbody fusion, or indeed as the Fischgrund paper [10] subsequently showed, instrumented.

Assessment of disability and outcome

The paper by Mannion [24] demonstrated that after a defined operation, decompression for root entrapment, the objective improvement in spinal flexion mirrored the improvement in self-reported disability. The group of patients was not typical of patients with disc herniation, their mean age was 57, and they had back trouble for a mean duration of 4.7 years. They were not patients with chronic back pain alone, they all had leg symptoms; they were a group of patients who had a disc herniation superimposed on a longer history of back pain. Usually in such patients their perception of disability is encumbered with much psychological and fear avoidance behaviour. The fact that a major component of their disability was effectively treated, and as a consequence their perception of their disability improved as well as their movement did, suggests that in chronic low back pain the psychological and illness behaviour not only develops as a result of the constant pain, but also our inability to cure the back pain with surgery is why the psychological problems persist. Another possible conclusion is that in a patient, who claims considerable pain disability in a compensation situation, and also has full spinal movements, is unlikely to be seriously disabled with pain. This is a very thought-provoking paper, and merits careful reading. Those interested in the question of back pain and movement in the sagittal plane should read also Burton [8], who deals with the fact that pain correlates well with movement if pain is present, but stiffness may be present, of course in the absence of pain.

A further paper by Mannion [23] concerning assessment of results of treatment is of great interest. Over the years, an enormous range of "instruments" to assess function and disability, especially in relation to outcome after surgery have been developed (Roland Morris Oswestry, WHO Quality of Life SF 36 etc.). Stimulated by Deyo [9], Mannion and her colleagues have come up with a set of questions, a six item core set of questions which would be practical for use in a wide variety of settings, including routine clinical care, quality management, and more formal research. They covered pain, functional disability well being, and satisfaction with treatment. They examined the test–retest reliability, validity, and responsiveness of the individual core measures. They found that they correlated well with the more massive

assessment instruments, and as they are completed by the patients, one is sure that compliance with this ''assessment form'' will be much superior to the more massive forms that have been introduced.

Suction drainage after surgery and epidural fibrosis

The paper by Sen et al. [30], which reported a prospective study on the effect of closed-suction drainage on the incidence of epidural fibrosis after discectomy was of great interest for a number of different reasons. The classification they used for evaluating fibrosis, combined with the MRI illustrations was easily understood, and certainly could form the basis in other studies of this subject. The fact that they did show a relationship between the degree of fibrosis and the clinical outcome was significant, insofar as most studies to date fail to show such an association, and this may be related to the precise nature of the classification of fibrosis they have used and the plan of their study. Reviewing the important paper by Annertz [4] published in 1995, which strongly questioned the significance of fibrosis in relation to symptoms and was very influential in altering our attitude to the significance of fibrosis and its relation to pain. Annertz compared postoperative symptomless patients with postoperative symptomatic patients, and found fibrosis equally common in both groups. They established that patients can be asymptomatic with fibrosis, but did not establish that in symptomatic patients, the degree and pattern of fibrosis may be a factor as to whether it causes pain. Because of their careful geographic classification of where the fibrosis is, the Sen group does establish that patterns of fibrosis can be related to pain. The other intriguing feature of this study was that closed drainage, gravity only, had such a seemingly significant effect of degree of fibrosis. The present enthusiasm for short stay may mean that patients, in whom early discharge is planned, may not have drains. I believe most surgeons in the UK do drain their wounds after a disc excision, mostly suction drains. It would seem this study confirms the use of that practice, and they can be simple gravity drains, which is relevant if one is also concerned about any dural injury.

Spondylolisthesis

The paper by Lamberg et al. [20] deals with the long-term results of surgery in adolescent patients. These were all patients with isthmic spondylolisthesis. They were all so called low grades—that is under 50% slip, and were a mixture of posterior fusion—L4-S1 and posterolateral fusion L5-S1. The paper has a great deal of important information and repays well with careful reading. All the patients had a degree of stiffness of their backs, but the two-level fusions were no worse than the single level. Overall functional levels were good, and in those who had a degree of disability, this did not correlate with the presence of a pseudarthrosis (ODI in the pseudoarthrosis patients was 9.6, and in the united group 7.6, the difference not being of statistical significance). Of great interest was the fact that adjacent level disc degeneration was only 12%, much the same as prevalence in healthy middle-aged individuals without spinal disease. This is much lower than the prevalence of disc degenerative changes in patients who have had an instrumented fusion for degenerative disease and low back pain. It certainly would support the view that adjacent disc disease is genetically determined rather than due to mechanical factors. These results again confirm that uninstrumented posterolateral fusion is the procedure of choice for this disorder.

One has to compare the above paper with that of Spruit et al. [34]. They report 12 patients with a follow up of 5–6 years. They had a wide decompression and a circumferential fusion using posterior pedicle screw instrumented reduction and staged anterior cage-assisted interbody fusion. Two patients were disappointed with the clinical result, and two patients had to have the screws removed due to prominence. Removal of the screws did not alleviate the patient's symptoms. One disappointed patient ''would refuse the staged surgical treatment'', because after all it had been exhausting and recovery was difficult. The authors advocate that a ''solid construct'' is necessary because of ''inherent instability'' of spondylolisthesis. The problem with this study is that the age mix causes confusion (22–54). The younger patients had symptoms due to the spondylolisthesis unclouded by degenerative disc disease, and are comparable to the study of adolescent patients mentioned earlier, the older patients essentially had degenerative disc disease, and as has been shown by Axelsson [6], they are no more unstable than other patients with degenerative disease. Using stereophotogrammetric techniques they showed that the adult isthmic spondylolisthetic does not cause permanent instability/hypermobility detectable in the adult patient with low back pain and low-grade olisthesis. The adult lytic spondylolisthetic is a patient with degenerative disc disease and surgery in this group has quite different results from surgery in the adolescent, presumably because the pain source is different. It is unfortunate that the presence of a lytic defect causes all such patients to be lumped together, irrespective of age, and surgeons are encouraged to operate on the adult spondylolisthetic in the hopes of the degree of success achieved in the adolescent, and not based on the results achieved in adult degenerative disc disease.

The paper by Remes et al. [26] dealing with the same group of adolescent patients from Finland described

earlier, in their paper looked at the MRI scans of these patients and correlated them with the clinical results. Within the group at follow-up, there were 9 who had an Oswestry disability index of more than 20, there were 93 whose ODI was 20 or less. Although disc degeneration at the level of the spondylolisthesis was present in 82%, and facet joint degeneration above the fusion was present in 79% of patients, these findings did not affect clinical result. Even the 28% severe facet joint degeneration was not symptomatic. It makes one consider whether facet joint degeneration is a significant factor in low back pain generally, and whether it really should be used as a contra indication to arthroplasty!

Low back pain

The paper dealing with discogenic pain by Hyodo et al. [18] was of interest. It suggested that acute low back episodes were due to annular tears in some 70% of patients, based on the response of local intra discal anaesthetic into discs, which on MRI showed such lesions. What was surprising was the high level of MRI changes. All had MRI changes, and 75% of them were grade 3 (Gibson scale). They describe the patients as having sudden onset of severe unendurable low back pain, without radicular pain, so one suspects this is a subset of very severe pain. Only 20% were related to a heavy lift, and some 30% had no obvious triggering factor, and some 45% were due to an ordinary casual movement. In the context of alleged work-related injury this is an important paper, indicating that radial tears are most commonly not due to an injury. On the other hand it does demonstrate that there is a relationship between an acute radial tear and severe pain. One cannot ignore the clinical description given occasionally by patients that they felt something tear, not only in the context of an event for which they are claiming compensation.

Insoles and back pain

The paper by Shabat [31] dealing with insoles challenges one's preconceptions concerning their value. This paper is a double blind prospective study comparing the effect of two insoles in relieving back pain occurring in patients who do a lot of walking. The insoles apparently look identical, but one is a custom-made, computer-generated one, and other is just an insole, but they look identical. The custom-made device was much more effective. One is always suspicious about a patented device, probably expensive; which claims it will cure back pain. One is even more suspicious about claims that an insole will do so. Yet, despite reading and rereading this paper it did seem that it was an appropriately blinded study. It may be that this is a particular group of people insofar as their occupation is concerned, and in the ordinary back pain sufferer there are different precipitating factors. The authors do not declare any financial interest in the device, so perhaps in this rather selected group of patients such an insole would be of value. It would be of interest to know what it costs and how independent the study was.

One common feature of the so-called mechanical low back pain is the pain experienced when patients bend forward to hoover, or wash their hair and the paper by Harrison et al. [14] which shows that with anterior thoracic translation, the strong muscle forces used in this position increase from 147 N to over 600 N? This is particularly at the L5/S1 level. It has always struck me as curious that in modest bending, although the most movement is in the upper lumbar spine, pain is felt at the lumbo-sacral level, where in this situation the amount of movement is minimal. When pain is related to the loading experienced at that level the reason is much clearer. It is an easily understood biomechanical paper, which has significant clinical significance, now that we increasingly recognize the vital part abnormal loading plays in back pain rather than the fact of movement alone. Movement may be an essential part of pain causation, as it causes or allows the body to move into a position of abnormal loading.

Kyphoplasty and vertebroplasty

Last year Heini et al. [15] in their review of the literature concluded that the case for kyphoplasty as being superior to vertebroplasty had not been made. The paper from Gaitanis et al. [12] dealing with some 32 patients in whom balloon kyphoplasty had been carried out, does not address this issue, but it does address the issue of methods of imaging prior to balloon kyphoplasty. The suggestion that use of the STIR sequence, which will show bone oedema is a better guide to the likelihood of a good clinical result, than a positive bone scan, in those patients in whom the procedure is being done some months after the fracture occurred. Their incidence of cement leakage was 10%, and this compares with an incidence of 81% reported in the paper by Schmidt [29]. This paper draws our attention to the three types of leakage, that through the fracture, that through the segmental vein-potentially leading to pulmonary and even cardiac complications, and that through the basilar vein, going into the spinal canal. Using CT scanning they demonstrate that the use of fluoroscopy and plain films seriously underestimates the leakage rates. Any leakage has potential for serious complications, and indeed in their series of 21 patients there were two serious neurological complications due to Type B leaks (basilar

vein). One presumes that such venous leaks are particularly related to the low viscosity of the injected cement, and the pressure under which it is injected. Balloon kyphoplasty in this regard with higher viscosity, and lower pressure may be safer in terms of the likelihood of vascular leaks. Your reviewer has seen asymptomatic patients with casts of cement in their heart and lungs, and suspects that the authors are right in supposing that many venous leaks are never recognized.

In his review of the paper by Gaitanis, Pellisé [25] makes the point that biopsy of the fractured vertebrae was an important part of management, and that the low success rate of 61% in the Gaitanis series was disappointing. The paper by Boszczyk [7] describing a transcostovertebral approach of the mid- and high-thoracic spine remedies this, as they achieved 100% success. Your reviewer has always used this approach for biopsy, and with a 2 mm needle achieved a similarly high rate of successful biopsy (Fyfe [11]).

The paper by Boszczyk describes the technique very fully, and is well illustrated. It has always been a surprise to me that the pedicular approach became so usual, as the ability to reach all parts of the vertebral body through one pedicle is severely constrained, the transcostovertebral approach for biopsy is certainly better, and can clearly be used safely for balloon kyphoplasty.

Fusion

The paper by Vaccaro et al. [35] is very topical. Essentially they found that compared with an historical control, BMP did not produce a higher rate of fusion, although overall there was a clinical success rate of 89%. This was of course a disappointing result after the very successful animal work that has been done concerning BMP. I was somewhat disappointed that the authors in their discussion did not discus the significance of this failure more fully. Is it volume of BMP used? Should the mix of BMP proteins be more extensive? Are the high hopes of BMP misplaced?

Fusion for low back pain achieves perhaps a 70% success rate, as does disc arthroplasty, so the conclusion by Maigne et al. [21] that based on sacroiliac joint blocks, 38% of pain after an unsuccessful fusion could be blamed on the sacroiliac joint is an attractive conclusion. Naturally one is concerned that the primary diagnosis before spine fusion was sacroiliac pain, but their figures did not support this view. Bone graft harvesting also was not found to be a consistent cause, as there was a lack of relationship between side of harvesting and the side of the pain. The lack of correlation between blocks and bone scintigraphy and even SPECT was disappointing. Although one regards a positive sacroiliac block as a strong indicator that the joint is the pain source, the fact that a second block is only successful in about half the patients clouds the issue further (Maigne [22]). It would be attractive if we could blame our fusion and arthroplasty failures on the sacroiliac joint, perhaps we can with some.

Paraspinal approach to the lumbar spine

Having used for some 30 years the approach (Wiltse) it might be thought that this paper by Vialle et al. [36] had little to teach me. However, I had often been struck by the variability in terms of bleeding and ease of dissection of the approach as I did it. This paper makes it all clear, that following the standard Wiltse technique, one may find the right plane, but one may not. This paper gives a very clear exposition of the various paraspinal approaches, well illustrated, and very clear. I would regard it as essential reading, now that pedicle screws are often inserted via this approach.

Spinal stenosis

Dynamic electrophysiological examination in patients with lumbar spinal stenosis

This paper by Adamova [1] reports a negative result, dynamic studies are not of value, although they demonstrate changes, such changes can be present in patients without claudication. Spinal stenosis, especially in the mild diabetic patient is a difficult diagnosis, and it is a shame that these examinations do not help. What was so valuable in this study is that it demonstrated the vital need in such studies for a control group as unless a control group had been included, the fact that changes were demonstrated in the spinal stenotics would have encouraged the belief that such investigations were of value.

The paper by Humphrey et al. [17] deals with the value of electromyography of the lumbar paraspinal muscle in discriminating between chronic low back pain sufferers and normal subjects. It has been established by the unit publishing this work, (who have a long involvement in the field) and by others, that EMGs in the spinal muscles differ between people with low back pain, and those without. Unfortunately an important discrimator, MVC, is influenced by load, and this affects many of the other apparent differences. MVC depends on the effort and motivation of the subject. Hence abnormal ECGs will be present in the ''normal malingerer'' as well as in the genuine patient, with pain. However, this paper does shed a ray of hope that we may in time be able to identify the malingerer from the genuinely disabled patient, by studying other aspects of EMGs made possible by the use of sophisticated computer software. It does seem that the spectral half width

and initial median frequency are discrimatory of the differences between a person with back pain and one without. Although to the nonspecialist, which includes your reviewer, the paper is hard going, it is a good example of the value of a unit pursuing in depth what is at the start a rather arcane study. It may be that the true usefulness of these studies is to be used to evaluate the changes achieved by various modes of training. One rather despairs at the present lack of scientific physiological evidence of the value of such physiotherapeutic techniques, such as ''core stability training'', as opposed to clinical evidence.

In summary there have been a wide variety of papers affecting management this year and our understanding of spinal problems, fully justifying the increasing size of the Journal.

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